in distribution of health care resources.[5] This type of research, dependent on epidemiology and economic metrics, often requires assistance with public health and business school faculty. Academic plastic surgery institutions are in a setting where a surgeon, epidemiologist, and economist can collaborate to identify cost-effective strategies for decreasing the global surgical burden of disease. For academic plastic surgery institutions, this would enable its faculty and residents to participate in innovative research and offer recommendations to policy makers on the management of plastic surgery issues.

The third role is aiding the development of local infrastructure for long-term sustainability. Medical missions have the potential to create a permanent, sustainable solution for decreasing the global surgical burden of disease with support from academic plastic surgery institutions. Academic plastic surgery institutions can obtain basic surgical supplies (e.g., wound vacuums, dermatomes) at discounted rates from vendors. Using the cost-effective methodology, academic plastic surgery institutions and local staff input can optimize the allocation of resources and the scaling of infrastructure for the long run. Furthermore, a relationship between academic plastic surgery institutions and local hospitals has the potential to garner credibility from the government and potential donors and engenders a teaching environment.

With surgery becoming more recognized as an integral tool in addressing the global health disparities, academic plastic surgery institutions will play a critical role in the reduction of the global surgical burden of disease. Incorporating these roles may improve the ability of academic plastic surgery institutions to produce excellent plastic surgeons and public health leaders armed to tackle the global surgical burden of disease.

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Introduction of the Implantable Doppler System Did Not Lead to an Increased Salvage Rate on Compromised Flaps: A Multivariate Analysis

Sir:

We have read with interest the past two issues of *Plastic and Reconstructive Surgery*, with both issues publishing studies that have investigated the usefulness of the implanted Doppler probe for monitoring free flaps.[1,2] This probe is unique among monitoring techniques because of its direct and instantaneous monitoring of pedicle status. Having published some of our own results and research with the use of this device, a combined analysis with these articles yields interesting findings.[3,4]

Paydar et al. have clearly had a positive experience with their use of the device, with an enviable salvage rate of 95 percent, which includes a significant proportion of buried flaps. Such success rates have been matched only by Kind et al. in their series of patients who were also monitored with the implanted Doppler probe.[5] However, Paydar et al. compared their cohort to literature values only, rather than any control group, such as other free flaps performed by their unit that were not monitored with the implanted Doppler probe. This makes statistical analysis of the effectiveness of their success impossible, and data from a similar series of cases that were performed by the same surgical unit and not monitored with implanted ultrasonic monitoring would be highly beneficial.

Smit et al. did made a direct comparison between flaps monitored by implanted Doppler devices and flaps that were not. Their research method is thus robust, showing an increase in their salvage rate from 60 percent to 69 percent, which they argued was not statistically significant ($p = 0.44$) and that therefore their study showed a lack of benefit for the use of the implanted Doppler system. With such a large magnitude in the improvement in salvage rate, it simply appears that the study performed was not sufficiently powered to establish the significance of the results. With greater numbers, significance of such results may yet be realized. This may be a more appropriate conclusion to that study.

There are now six studies of this device that have all shown salvage rates of 69 percent or higher (the others

showed 80, 83, 95, and 100 percent).[1–6] Considering this homogeneity of the published data, we think that this device has indeed proven useful, particularly in the setting of buried flaps, which were previously not monitored at all.

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Sequential Heart and Composite Tissue Allotransplantation in Rats

Sir:

I read with great interest the article entitled "A Model of Sequential Heart and Composite Tissue Allotransplant in Rats" published by Yang and colleagues in July of 2010 (*Plast Reconstr Surg.* 2010;126:80–86). In this article, the authors estimate "600,000 patients with solid organ allotransplants currently in need of reconstruction with a composite tissue allotransplant." With this in mind, they were quite clever in their initiative and should be congratulated for investigating the role of composite tissue allotransplantation following solid organ transplantation.

However, I have a few additional concerns regarding this article. First, their study design was encouraging and novel, but its potential to provide translational value will require adjustment in the future. For instance, recipients received 10 days of cyclosporine following their heart transplantation, and then immunosuppression was discontinued following abdominal wall transplantation on postoperative day 10. Therefore, the only data gathered were from heart and abdominal wall allotransplants assessed/monitored on the way to inevitable acute graft rejection. Although the objective of Yang et al. "was to assess rejection severity and not to prevent rejection as would be done clinically," the authors' study will be much more insightful when it includes a second induction phase, and when daily maintenance therapy is continued long term for the evaluation of any ill effects related to the sequential transplant (i.e., graft rejection, opportunistic infection, immunosuppression-related complications).

In addition, it would have been extremely worthwhile had the authors discussed the second induction immunotherapy phase (which is required with sequential composite tissue allotransplantation) as both a major ethical *and* immunologic obstacle. This requires exposing fragile patients, who have already had successful outcomes from their life-saving solid organ transplant (i.e., liver, heart), to a variety of potential complications. Should we be risking primary solid organ graft failure with the transplantation of a life-enhancing composite tissue allotransplantation (i.e., face, hand, abdominal wall)? In other words, what is the risk-to-benefit ratio of exposing these recipients to additional antigens from a third donor?

In addition, I feel compelled to acknowledge work performed during my year-long surgical research fellowship at Robert Wood Johnson Medical School at Camden/Cooper University Hospital under the direction of Charles Hewitt, Ph.D., in which I also investigated heterotopic heart transplantation for the exact same reasons. In fact, this project received the "Best Overall Resident/Fellow Research Award" at the annual American Society for Reconstructive Microsurgery meeting in 2006.[1] It is therefore quite puzzling to me why the authors chose not to include and/or reference our two resulting publications,[2,3] one of which described our femoral model used to study acute graft rejection (a study comparable to the one presented here) (Fig. 1), *and* the other of which illustrated our novel use of transfemoral echocardiography to monitor graft rejection (Fig. 2).

In conclusion, the study by Yang et al. was very interesting and well timed, especially because there are numerous institutions throughout the United States